# THE JOURNAL

OF THE

# ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Honorary Fellow

No. 152 AUGUST, 1923 VOL. XXVII


## NOTICES

### *Associate Fellowship Examination*

The second examination for Associate Fellowship will take place in the Library on Monday, September 24th (Part I.), and Tuesday, September 25th (Part II.). Intending candidates should forward entry forms (accompanied by the prescribed fee) on or before Monday, August 27th, stating the subjects in which they desire to be examined.

### *Lectures*

The following programme of lectures has been arranged for the 59th Session commencing in October:—

| *Date.* | *Author.* | *Lecture.* |
|---|---|---|
| 1923. | | |
| October 4th ... | Chairman's Inaugural Lecture | (Subject to be announced later.) |
| ,, 18th ... | Sqn. Ldr. Hill ... ... | "The Manœuvres of Inverted Flight." |
| November 1st | Major Wimperis ... ... | "Present Developments in Aircraft Instruments." |
| ,, 15th | Mr. H. R. Ricardo ... ... | "The Thermodynamics of Aircraft Engines." |
| ,, 29th | Sqn. Ldr. Maycock ... ... | "Airmanship at Sea." |
| December 13th | Colonel Edmonds ... ... | "Air Strategy." |
| 1924. | | |
| January 10th... | Dr. Aitchison and Mr. North | "Materials from the Aeronautical Point of View." |
| ,, 24th... | Dr. Ramsbottom ... ... | "Fabric and Dopes." |
| February 7th... | A Representative of the University of Tokyo... ... | (Subject to be announced later.) |
| ,, 21st... | Mr. H. Hamshaw Thomas ... | "Aerial Photography and Survey." |
| March 6th ... | Major Tucker ... ... | "Sound Detection." |
| ,, 20th ... | Captain W. S. Farren ... | "The Report of the Aeronautical Research Committee's Panel on Scale Effect." |
| April 3rd ... | Colonel the Master of Sempill | "The British Aviation Mission to the Imperial Japanese Navy." |

XUM

### *Library*

The following books have been received and placed in the Library:—Southport Auxiliary Observatory, Annual Report, 1921, Air Ministry; Report of Government Financial Assistance to C.A.T. Companies, Air Ministry; Rigging Notes, Avro Biplane, Type 504K, Air Ministry; Universal Camera Mounting, Air Ministry; Napier Lion (Series II.) Aero Engine, Air Ministry; Notes on Giant Aeroplanes, by J. Weiss and A. Keith; Fatigue of Metals, C. E. Stromeyer; Design of Commercial Airplanes, E. P. Warner; Flying Fishes and Soaring Flight, E. H. Hankin; Commercial Aeronautics in China, H. Chatley; Radiators for Aircraft Engines, S. R. Parsons and D. R. Harper; Malaises des Aviateurs, de Brichambaut and Behague; Atoms, J. Perrin; Scientific Papers of John Aitken, Cambridge University Press; L'Aptitude au vol en Avion, P. Perrin de Brichambaut; Gliding and Soaring Flight, J. B. Weiss; Dictionary of Applied Physics, Volume IV., Light, Sound, Radiology, edited by Sir Richard Glazebrook.

W. Lockwood Marsh, *Secretary.*

# *SOME NOTES ON SIR GEORGE CAYLEY AS A PIONEER OF AERONAUTICS

BY J. E. HODGSON.

I propose to deal with the subject of this paper under three headings: First, a brief summary of Cayley's life and character; second, an account of his work on navigable balloons; and third, a like account of his work on mechanical flight. The latter sections will be prefaced with a short survey of the state of those two branches of aeronautical science at the period of Cayley's youth. It should, perhaps, be added by way of introduction, that Cayley's place as a pioneer in aeronautics has not yet been fully assessed. His name is barely mentioned in that greatly over-rated, ill-arranged and cumbrous compilation, "Astra Castra," published by Hatton Turnor in 1865, and it is entirely omitted from the "Dictionary of National Biography." In English aeronautical histories—for the most part regrettably unscholarly and inadequate—the comprehensive character of Cayley's work is usually overlooked, and though he is more frequently mentioned in terms of respect by French writers, their accounts are inevitably incomplete. For with exception of the excellent reprint of the essay on "Aerial Navigation"—dealing mainly with mechanical flight—published in 1910 by the Royal Aeronautical Society as the first of a series of "Aeronautical Classics," Cayley's writings—notably his investigations into the problems underlying navigable balloons—are not readily accessible.

## *Notes on Cayley's Life*

Sir George Cayley—he was the sixth baronet since the creation of the title in 1661—was born at his ancestral home Brompton Hall, in the North Riding of Yorkshire, on December 27th, 1773. The bent of his mind towards scientific pursuits became apparent in early years, and it is said that his interest in aerial navigation—"a noble art" as he subsequently termed it—was inspired when he was not more than 10 or 12 years old by the Montgolfiers' discovery of the balloon. As he himself relates his first experiment in aeronautics was made with a Chinese "flying top" in the year 1796, and it is significant of his steadfast attachment to the subject, that as late as 1854, he sent a description of an improved type of this aerial toy to the distinguished French aeronaut and writer, Dupuis-Delcourt. In 1795 he married Sarah Walker (the only daughter of Rev. George Walker, F.R.S., and President of the Literary and Philosophical Society of Manchester), by whom he had a large family, the members of which in later years were taken for extended tours to France and Italy. As a young man he was influenced—like Wordsworth and many more of his contemporaries—by the outburst of the French Revolution, and he took an active part on the Whig side in county politics, acting for many years as President of the York Whig Club. But though inspired with warm feelings in the cause of liberty, he was not drawn into sympathy with any unconstitutional proceedings, and in the spirit of a true Englishman, he took his part as the commander of a corps of volunteers when Napoleon threatened this country with invasion. His first published contributions on "Aerial Navigation" (mechanical flight) appeared in Nicholson's "Journal of Philosophy" in 1809-10; in 1816-17 he expounded in the pages of Tilloch's "Philosophical Magazine" his ideas on navigable balloons, a subject to which he returned in letters published in the "Mechanics' Magazine" in 1837.

* A Paper read before the Newcomen Society and published by arrangement with that body.

Further contributions (suggested by the discussion on Henson's "Ariel Carriage") to the latter periodical in 1843, with two articles in the Bulletin of the "Société Aérostatique et Météorologique de France" in 1853, complete the record of his extant writings. It can hardly be doubted that these cover but a small part of all the investigations, calculations and experiments made on aeronautical matters over many years. Of his life and scientific labours apart from aeronautics little is known. Politics and the affairs of the family estates doubtless occupied much of his time, but it is clear that his chief concerns were science and engineering in general, and aeronautics in particular. He built a shed or outhouse at Brompton in which he conducted a large number of experiments on the application of electricity as a motive power, and on the development of the gas engine. In the latter direction he contributed to Nicholson's Journal in 1807 a "Description of an engine for affording mechanical power from air expanded by heat," but though constructed at Newcastle on what Cayley termed "a considerable scale," it proved unsuccessful.

Later in life he accepted the congenial office of chairman of the old Polytechnic Institution, while in 1852 he stood successfully as Parliamentary candidate for the borough of Scarborough. But at his advanced age the duties and responsibilities of a Member of Parliament must have proved arduous, and it has been suggested that he only entered Parliament in order to draw public attention to a subject which, rightly and with remarkable foresight, he realised to be of the greatest importance to this country and to civilisation at large. He is not known, however, indeed he is not likely to have found opportunities of impressing his views on the House. In any case he retired after two years and died at Brompton Hall on December 15th, 1857.

Of Cayley's character it is difficult, from the meagre knowledge available, to form a true conception. I regret it is not possible to do so, for as one whose studies have mainly concerned men of letters and men of action, I know the value of a just and lively estimate of a man's character, as a help to understanding his work and his aims. Engineers and inventors are just as human as other men, and their characters are surely worthy of, and should receive, a due measure of attention. From the little we do know of Cayley, I imagine that his temperament and environment tended to make him a genial country gentleman—one blessed with ample means, with a family to rear and educate, an estate to manage, and local affairs to engage some measure of his time—rather than to develop an engineer or a scientist, whose whole life was destined to be sternly devoted to one or other of those fields of endeavour. At the same time I feel that the late Sir Walter Raleigh, in the necessarily brief account of Cayley contained in the introductory chapter to his "History of the War in the Air," was a little depreciatory in suggesting that Cayley merely "amused his leisure with science," as also in his remarks that Cayley "put nothing on the market," and was "content to enunciate a truth and to call it probable." For Cayley's work, as we shall see, was seriously undertaken, though I gather he had no ambition to earn rewards, either material or otherwise. He himself disavowed any desire "to scramble"—as he called it—for any share in the credit, at that time too often mistakenly attached to the invention of aerostatic machines, "save only," he significantly added, "that of braving the risible muscles of my friends by substituting *acres* for *yards* of cloth in their structure." Indeed the moderation he exercised in setting forth his ideas—indicative of a certain measure of greatness—affords a marked contrast to the assurance frequently displayed in contemporary ballooning projects. Nevertheless, he sought to encourage the work of others—his criticism of Henson's "Ariel Carriage," for instance, was not less restrained that it was sound—and in his eagerness to acquire information he carried on a considerable correspondence with scientists and inventors of his day, both at home and abroad. I have read that it was due to Cayley's generous

financial help that his friend, Baron de Ferussac, was enabled to start the publication of the "Bulletin Universelle." One can only add that the amplitude of Cayley's views on aerial navigation suggests a mind of great breadth of view, endowed with remarkable imaginative foresight. For as early as 1809 he expressed complete confidence in the practicability and security of mechanical flight as a method of transport, at a speed exceeding that of the railway train, and by 1816-17 he was advocating the navigable balloon with even stronger conviction, as offering "a direct, swift and easy floatage from any one point to every other on the face of the globe." His large conception of air travel cannot be more forcibly expressed than in his own words. "An uninterrupted navigable ocean," he said, "that comes to the threshold of every man's door, ought not to be neglected as a source of human gratification and advantage."

### *State of Contemporary Aerostatic Science*

I pass to outline, as briefly as possible, the state of the science of aerostation, particularly with regard to the direction or control of balloons, at the time when Cayley first turned his attention to the subject. It is well known that the balloon was invented by the brothers Montgolfier in 1783, and that in October of that year Pilâtre de Rozier first ascended into the air in a balloon of the Montgolfière type—that is a balloon inflated with hot-air rarefied by means of a fire burning in a brazier. In the following December this method of inflation was improved upon by J.-A.-C. Charles, who introduced the use of hydrogen. After hundreds of years of aspiration and fruitless endeavour, these achievements were naturally regarded with wonderment and gave rise to extravagant hopes of utility in the service of man. But it was realised almost at the outset, that the use of the balloon as a method of aerial travel would be greatly limited, unless it were possible to devise a method of control. The case was aptly put by a contemporary versifier in four lines:—

> "To Montgolfier the invention's due
> Unfinished as it lies,
> But his will be the glory
> Who direction's art supplies."

Hence arose countless schemes, mostly emanating from France, conceived to accomplish that obviously desirable object. By a natural but erroneous process of inventive thought, the false analogy of the boat sailing on the water and steered by a rudder, was followed in the earliest projects. One of the first designs embodying the sails and rudder principle was that of Thomas Martyn, a natural history draftsman, who published in London an engraving of his "Aerostatic Globe," which he claimed to have originally designed in November, 1783. Such suggestions can have served no other purpose than that of making clear the apparent lack of what was then commonly termed a "point d'appui," and, as a corollary, the need of applying a propelling force. Oars or wings, consisting of a light framework covered with silk or other material, were the first forms of mechanism designed to afford manual propulsive power, J. P. Blanchard, in March, 1784, being the first to experience—though not to admit—their inefficiency. Other projects tried or suggested were forms of jet propulsion (by means of hot-air, on the principle of the æolipyle of the ancients), or by the reaction of gunpowder exploded in the form of rockets. Yet another method—invented by David Bourgeois in 1784, which also engaged the attention of Montgolfier in France, and (at a slightly later date) of R. L. Edgeworth and Cayley himself in Great Britain—involved the use of an adjustable plane surface fitted beneath the balloon, whereby to obtain some measure of control from the pressure of air on such plane during the rise or fall of the balloon. Of the great majority of these early schemes it need only be said that they were faulty in theory or futile in application. An important exception must be made, however,

in the case of the theoretical examination of the principles involved, undertaken by J.-B.-M. Meusnier, equally distinguished in military, scientific and mechanical achievements. As early as December, 1783, he prepared a memoir in which he suggested the need of making the form ellipsoidal, and introduced the use of the air ballonet. It is probable, however—I am indebted on this point to my friend Colonel Lockwood Marsh, Secretary of the Royal Aeronautical Society—that Meusnier erroneously conceived the ballonet as a means of height control rather than for the purpose it more truly serves in modern practice—namely, to preserve the shape of airships of the non-rigid type. But Meusnier's work and the drawings and plans which accompanied it, are very remarkable, as well in respect of form and the ballonet, as in his ideas of diagonal rope suspension of the car and in the application of airscrews—worked, of course, by manual power—for propulsion. The airship—it deserves to be so termed, if only for the fact that it was designed to be 260 feet in length—was never constructed, and the only contemporary test of the ballonet principle was in its profitless application to the elongated free balloon of the brothers Robert, in which they made ascents during July and September, 1784. An air ballonet was, however, incorporated in the design of a "fish-formed" balloon invented in 1789 by Baron Scott, a French officer, who may possibly have taken his idea of shape from the so-called "Flying Fish" aerostatic machine exhibited in Cornhill in 1785, which there is reason to believe was the work of John Hoole, son of Samuel Hoole, watchmaker and mechanician, and (more notably) the friend of Dr. Johnson. Incidentally, it may be added that Johnson not only gave expression, at an early date, to doubts on the utility of the balloon—"a species of amusement," as he characterised the invention in a letter to Dr. Brocklesby, "for I do not find that its course can be directed, so as that it should serve any purpose of communication"—but agreed that wings would not assist as a means of direction. Scott's ideas more probably inspired S. J. Pauly, a Genevan gunsmith, who actually constructed a "fish-formed" dirigible which he tried with some measure of success at Sceaux in 1802, deriving an inadequate propelling power from the use of wings or revolving oars. In 1815 he came to London, and with the financial help of Durs Egg, doubtless known to many of you as a noted London gunsmith, the "Dolphin Balloon" was commenced at Kensington. The project has an indirect connection with Cayley—whose published writings on dirigible balloons it preceded—inasmuch as on hearing of it, and realising the great expense of such ventures, he reaffirmed the need of conducting experiments by means of public subscriptions. The envelope—about 90 feet long—was made of layers of gold-beaters' skin, and it was to contain an air ballonet of 21 feet in diameter, horizontal stability being maintained by means of an adjustable weight suspended between the tail of the balloon and the car. A more important feature was the intention to obtain "the propelling impetus" from "a kind of atmospheric steam engine, invented by Mr. Collier." But financial and other difficulties proved insuperable, and "Egg's folly," as it was called by the cynics, was never completed. It represents, however, in the main, the stage the navigable balloon had reached in Cayley's day, the chief features being the shape—tending towards streamline—the air ballonet to preserve that form, the suspension of a gondola or car beneath the envelope, and the suggested use of steam as a prime mover.

It will of course be appreciated that the foregoing remarks are but the merest outline, and that there were many aspects of the problem which, in those early days, had received but small consideration at the hand of the pioneers. These men aimed primarily at using the principle, the "floatage" as Cayley called it, of the balloon, as an aeronautical device, and by a modification of the original shape and the application of motive power, rendering it capable of direction and control, thus converting it into a machine fitted to the purpose of aerial navigation. The highly complex and multifarious questions in physics, mechanics, metallurgy, and so forth, with which the airship designer and constructor of

to-day has to deal, were, perhaps fortunately, beyond the ken of the pioneers. They strove with such elementary knowledge as they possessed to do no more, but no less, than drive and steer a balloon through the air—an achievement which was not actually accomplished until 1852, when Henri Giffard, in an elongated balloon fitted with a steam engine of three horse-power, obtained an independent speed of six miles per hour.

### *Cayley's Work on Navigable Balloons*

Coming to Cayley's own work on dirigible balloons, it has been already mentioned that his first published essays on the subject appeared in Tilloch's "Philosophical Magazine" during 1816-17. It is true that his earliest writings on aeronautics dealt with mechanical flight, but I take his contributions to "lighter-than-air" theory first, because his ultimate faith in the success of aerial navigation over the world's surface was based on the possibilities of the navigable balloon. He made this clear beyond dispute in one of his last letters on the subject. He pointed out that it had been proved—to use his own words—on "tolerably well-ascertained data," that elongated balloons of a large size were capable of being driven through calm air at a speed approaching that of the railway train, and could carry a considerable cargo by reason of their buoyancy. From these premises he argued that "on a great scale, balloon floatage offers the most ready, efficient and safe means of aerial navigation." "Elongated balloons of large dimensions," he wrote in the same paper, "offer greater facilities for transporting men and goods through the air, than mechanical means alone, inasmuch as the whole weight is suspended in the air without effort . . . and when the invention is realised, it will abundantly supply the increasing locomotive wants of mankind." Moreover in the sentence immediately following he gave prophetic utterance, with remarkable foresight, to the view which, after much ebbing and flowing, has in quite recent years received the support of distinguished aeronautical experts—the view that the relation of airships to aeroplanes is complementary and not competitive. "Mechanical flight," Cayley wrote in 1843, "seems more adapted for use on a much smaller scale, and for less remote distances; serving, perhaps, the same purpose that a boat does to a ship, each being essential to the other."*

Broadly speaking, it was Cayley's ability to grasp the basic scientific or mechanical principles underlying the theory of navigable balloons, rather than his skill as an inventor or designer, wherein lies his true greatness as a pioneer of the airship. He was, for instance, one of the first to realise fully the practical significance of one of the main factors on which airship theory rests—namely, the physical law that "the surfaces (and hence the resistance) increase as the squares of the diameter of the balloon, whereas the capacity to contain gas (and hence the supporting power) increases as the cubes of the diameter." If it were not for that principle he knew quite well that the difficulties would be far greater; or, as he himself put it, "we must be contented to give up balloons for purposes of locomotion altogether, or to attempt them on that scale of magnitude which a well-grounded calculation of their power proves to be necessary." As to the factors involved in the question of resistance, he pointed out (as indeed others had already done), that obviously the spherical form of the ordinary free balloon should be lengthened horizontally, thus diminishing the cross-section for the same volume, and he realised the desirability of dividing the gas into several compartments—as he said, "like the stomach of a leech." With more originality he

* The importance Cayley attached to the advantages of the airship is revealed in the fact that while he first stated them in the "Mechanics' Magazine," March, 1837, he not only reprinted them (as an interpolation), but enforced them in the final paragraph of his essay on mechanical flight, contributed to the same magazine in April, 1843.

even suggested that dirigibles, " when used as permanent vehicles and on the true scale of magnitude, will probably be made of thin metallic sheets kept firm by condensation [that is pressure] with separate light bags of gas within." For a first experiment on these lines he suggested that Charles Green's large balloon —presumably the famous " Nassau Balloon " of 1836—should be requisitioned, and that two other balloons of smaller size should be " packed at opposite sides of this larger one," a suggestion which, however, was not tried. Of greater interest is his early anticipation of some degree of rigidity, revealed in the proposal to guard against the then unknown laws of resistance offered by fluids to solid bodies, by means of " light poles and internal cross bracings of wire or cord," designed to preserve the shape of his elongated spheroidal balloon. Moreover Cayley carefully considered the problems affecting the transmission of power from an engine suspended in a car beneath the envelope to the balloon itself—a point of particular moment in his own design owing to the need of keeping the boiler and furnace of the steam engine which he proposed to fit as far as possible below the envelope. As to his " prime mover," he had perforce to adopt the steam engine, but it is, I think, quite clear that he realised it was by no means an ideal form of power for the purpose, and there is reason to believe that his experiments, both with gas engines and electricity, were largely inspired by the idea of turning those sources of energy to account in aeronautics. With regard to the fabric of the envelope, which necessitated, as Cayley laid down, a material " perfectly air-tight, light and strong," he suggested that the great expense of silk (covered with india-rubber varnish) would be prohibitive, and proposed as an alternative " double-cotton Indian-rubber cloth," as invented in 1823 by Charles Macintosh for waterproof garments and air-tight cushions. In this connection it is of interest to note that Cayley's work was not wholly theoretical, for in 1816 he refers to a cloth weighing ½lb. per square foot as used in " my experiments."

It is not possible within the limits of this paper to enter on any full account of Cayley's designs for a navigable balloon. It must suffice to give some indication of his ideas in general, and the calculations (in all cases quoted from his own writings) on which they were based. In his first design, as communicated (with accompanying plans) to Tilloch's Philosophical Magazine in 1816-17, he provided for a Montgolfière or hot-air balloon 300ft. long, 45ft. in elevation, and 90ft. wide, made of " woollen cloth." In form it was an elongated spheroid, with a conical head and a slight tapering towards the stern—on the axiom common among sailors, that a ship to sail well should have a " cod's head and a mackerel's tail "—and it was to be kept to its shape by the light poles and cross bracing before mentioned. Professor Raleigh has remarked on the soundness of Cayley's ideas on what is now known as the doctrine of streamline, but he points out that though Cayley realised that the shape of the hinder part of a solid body travelling through the air is of as much importance as the shape of the fore part, he does not seem to have known that it is actually of more importance. In the first design the impelling power was to be derived from the deviation obtained from the pressure of air on a passive plane surface, a wholly inadequate method, the idea of which, as Cayley admits, was derived from the plan described by John Evans a year earlier. In his next essay, Cayley goes far beyond his original ideas, and offers calculations based on the possibility of propelling an elongated balloon by wing waftage—that is, wings fitted to the sides of the car and actuated by mechanical power. He expressed a preference for this (mistaken) form of propulsion as against " rotary wafts " or airscrews, mainly owing to the difficulty of " giving firm support and communicating motion to the latter," though admitting the advantage they afforded of uniform action. This improved design was to be 144ft. long, with a lift of 163,000 lbs., which reduced by the weight of the materials (1,700 lbs.) and of the engine, boiler, fuel, etc. (15,210 lbs.), would leave about 34 tons. Stated in terms of performance, Cayley estimated that this would allow of the transportation of 50 men for 48 hours, or a voyage of 960 miles

in calm air. Further consideration of this "balloon of 50 tons" led him to comment on the difficulties involved in the "stupendous bulk"—difficulties arising from expenses of construction and inflation (he estimated hydrogen to cost £300 a ton, though looking to a reduction by the application of new methods of production) and difficulties in "disposing of them when not employed." He does not appear to have contemplated the construction of immense airship sheds, such as have been erected in recent years, for he made calculations on the "horizontal drag" of large balloons at anchor, from which he deduced that this drag diminishes with size and oblong structure. Indeed, at a later date he laid down that "permanently-filled balloons would ride out storms when properly secured, without the danger of being driven to the earth or damaged."

In 1837 he revised the foregoing speculations in a lengthy communication to the "Mechanics' Magazine," wherein he sought to present considerations which would be "most conducive at present towards a final accomplishment of the aerial object in view." His elaboration of the "inclined plane" method of propulsion —of which he offered what he called "a rough and hasty sketch" by way of a plan—involved the combination of a large Montgolfière below and a smaller hydrogen balloon above, separated by the plane. But apart from the danger of such a combination—a danger experienced with fatal results in 1785 by Pilâtre de Rozier —the method was not worth even the brief consideration Cayley gave to it. Moreover, the twenty years or so which separated his essays had resulted in improvements in the steam engine, which by 1837 gave promise of greater power for weight. In this connection Cayley referred to the steam carriage of his friend Sir Goldsworthy Gurney (he wrote of it in 1837 as "recently completed"), the engine of which he calculated would give one horse-power for 200 lbs. weight. But though he recognised that "lighter first movers than steam engines may be discovered, and made applicable to propelling balloons," he proceeded to take the case as he found it. With a hydrogen balloon of a similar shape to his original design, 90ft. in diameter and 315ft. long, he estimated there would be available for engine power, crew and cargo, a lift of about 29¼ tons. He further estimated that a balloon of this size, bearing a strong resemblance, as he put it, "to a hundred-gun ship," would require an engine of 60 horse-power.

As to the application of propelling power, he still adhered to the idea of wings arranged in two tiers, but he also reverted to the idea of airscrews—"oblique vanes," as he said, "reversing the action of the sails of a windmill." As to the latter, he instanced the results of experiments made by the French Academy, which went to show that "a proper fulcrum or resistance for the engine power to work upon can be had at a velocity of 25 feet per second." Finally, reckoning the engine, with fuel and water for four hours, at 510 lbs. per horse-power, and deducting also the weight of the "machinery for waftage," Cayley arrived at the conclusion that travelling at the designed speed of 14 miles per hour his balloon would have a useful load of about 9 tons.

Cayley never attempted the construction of a large navigable balloon, but we may well believe he was only deterred by reason of the great expense. This difficulty led him in 1816 to suggest experiments by public subscription, an appeal he renewed in 1837 in the form of a proposal to organise a "Society for Promoting Aerial Navigation." It was doubtless the indifference shown towards his proposals that led him to write in 1843—in words which, with some modification, are even to-day not wholly inapplicable to the prevailing attitude towards the airship—"I think it a national disgrace in these enlightened locomotive times not to realise by public subscription the proper scientific experiments, necessarily too expensive for any private purse, which would secure to this country the glory of being the first to establish the dry navigation of the universal ocean of the terrestrial atmosphere."

### *Contemporary Ideas on Mechanical Flight*

Turning to the general conceptions of mechanical flight in Cayley's time, and the ideas which had led up to them, it may be said at once, that there were only two points of view—the one from which flight was regarded as a matter of flapping wings, and the other as a matter of impossibility any way. The old, the very old idea of wings, was doubtless inspired by the natural, not to say obvious, analogy of the flight of birds, a sense in which the practicability of the thing was first expressed by Roger Bacon in the middle of the 13th century, while early in the 16th century the idea occupied the great mechanical genius of Leonardo da Vinci. Indeed, broadly speaking, it is true to say that flight was invariably regarded in the light of the imaginary achievement credited by Francis Bacon to the inhabitants of his "New Atlantis," 1627. "We imitate also the flight of birds," says the sage, in a marvellously prophetic recital of countless mechanical devices and scientific notions practised in that Utopian community. Those words held good for something like the next two hundred years, not only, indeed, as an expression of aims, but as a baneful conception which blocked other fields of speculation. For that reason it is unnecessary to enter into any detail, or to recall the endeavours—more numerous, probably, than is commonly realised—made during the 18th and 19th centuries to achieve flight by means of wings. Even in the years immediately preceding the discovery of the balloon, J.-B. Blanchard was engaged in futile and fruitless attempts to construct a "flying vessell." Not that the study of the principles involved in bird flight was in itself profitless; on the contrary, Lilienthal himself regarded it as "the basis of aviation." But up to Cayley's day, and long after, those principles were not understood; they gave rise to the most varied and impossible speculations, and the blind attempts made to imitate their apparent characteristics diverted attention from the more strictly mechanical aspects of flight. It is because Cayley was amongst the first to approach the problems of flight from the mechanical and not, so to speak, from the ornithological point of view, and because he first conducted "gliding" experiments on a considerable scale, that he deserves to stand among the great pioneers of aviation in the direct line between such names as Leonardo da Vinci in the 16th, and John Stringfellow in the 19th century.

### *Cayley's Work on Mechanical Flight*

It has been seen that Cayley's first experiment in such matters was made as early as 1796 with a Chinese or aerial top (identical with the device exhibited before the French Academy of Sciences in 1784 by Launoy and Bienvenu), which served at once to illustrate the principle of the helicopter and the airscrew. Though but a toy of a few inches in length, its capacity to demonstrate certain elementary but important principles in aeronautics, made a lasting impression on Cayley's mind, and (as already mentioned) only three years before his death he sent to Dupuis-Delcourt a drawing of one which he had made—the best, he said, that he had ever seen, capable of rising 90 feet in the air. Having collected a body of "facts and practical observations in the course of much attention to the subject"—to use Cayley's own words—he published his first essays "On Aerial Navigation," dealing with the subject wholly from the point of view of mechanical flight, in the pages of Nicholson's Journal during 1809-10. The character of his observations and experiments is at once shown in his ability to grasp essential principles. His reflections on bird flight led him to the belief—confirmed by so great an observer as Charles Darwin nearly 25 years later—that flying required less exertion than was then commonly supposed. But he categorically denounced the idea of flight by means of wings (worked by muscular effort) as ridiculous. In his clearly expressed conviction that mechanical flight was possible, and in his enunciation of the "whole problem" as contained in the simple but comprehensive formula, "To make a surface support a given weight by the application of power

to the resistance of air," he is revealed as the earliest true pioneer of the aeroplane. He had perfect confidence in the practicability of transporting passengers and goods—the latter word is expressive of his large ideas—" more securely by air than by water, and with a velocity of from 20 to 100 miles per hour."

His earlier experiments in aerodynamics had revealed (the figures quoted are Cayley's calculations) that a surface of one square foot moving at a velocity of 11.538 feet per second generated a resistance equivalent to 4 ozs., or at 17.16 it gave 8 ozs. With some such data—obtained by an early form of " whirling table "—he proceeded to make " gliding " experiments, carried out over a number of years, on what he called " a considerable scale of magnitude "—apparently with a machine having a surface of 300 square feet—and his enthusiastic description of one of the trial flights (made from the high ground behind Brompton Hall) may be quoted as the first of its kind (and therefore historic) and as in itself of great interest. " It was beautiful," he wrote in November, 1809, " to see the noble white *bird* sail majestically from the top of a hill to any given point of the plain below it, according to the set of the rudder, merely by its own weight, descending in an angle of about 18 per cent. with the horizon." Cayley states that the upward lift of this " gliding " machine was at times so strong that anyone running forward in it against a light breeze would be raised from the ground for " several yards together."

But even at this date Cayley's thoughts were also engaged on the question of propulsion, or the necessity of a " first mover " as he termed it. Realising that a steam engine of the type invented by Boulton and Watt would be inadequate, he looked more hopefully to the development of some such engine as was reported to have been designed by William Chapman of Newcastle—an early form of internal combustion engine, with oil of tar as fuel. Evidently he intended to make some trial of a " propelling apparatus " with his " glider," for he refers to the fact that an accident prevented his doing so. He continued, however, to consider many other factors involved in mechanical flight—problems dealing with questions of initial velocity, the leverage on the wings, and the need for lightness in construction combined with strength. The latter he suggested for the first time might be achieved by designing superposed surfaces, as now usual in the biplane. The wings he conceived should be supported by " diagonal bracing " —which he termed " the great principle for producing strength without accumulating weight "—while he foreshadowed the necessity for streamline design in the maxim, that " in the art of aerial navigation every pound in direct resistance that is done away with will support 30 lbs. of additional weight without any additional power."

On the publication in 1843 of particulars of Henson's " steam carriage," he returned to the subject of mechanical flight, and further explained his ideas in two letters to the " Mechanics' Magazine " in April, 1843. His criticism of Henson's scheme, though accompanied at the outset with an expression of encouragement, clearly indicated his reasoned doubts as to its success. In the first place " the magnitude of the proposed vehicle," involving a terrific stress on the necessarily light structure of the main supporting surfaces or wings—150ft. span and 30ft. chord, as designed—afforded ground for serious misgivings. The stress or " leverage " on the wings Cayley again suggested might be overcome by securing the required surface not " in one plane, but in parallel planes one above the other," and he went so far as to propose a tri-plane, or " three-decker " as he termed it. Moreover, while expressing his conviction " that the inclined plane, with a horizontal propelling apparatus, is the true principle of aerial navigation by mechanical means," Cayley doubted Henson's ability to provide the " very great engine power—the *sine qua non* of the case "—which the design required.

As in the case of navigable balloons, Cayley's design for an " aerial

carriage" (which he described in the same paper) is hardly as interesting, and certainly not as sound, as his examination and discussion of theoretical principles. His plan shows that instead of obtaining the required supporting surface by means of rigid horizontal wings, he proposed two sets of superposed circular planes (designed when in motion to act as helicopters) set at an obtuse angle, and revolving in contrary directions. These circular planes he termed "elevating fliers" to distinguish them from two smaller horizontal airscrews for propelling the machine. The "framing" or fuselage was to be covered with canvas at once to increase the surface and afford protection to the engine, while a "broad horizontal rudder or tail" was designed for use in ascent or descent, and to act as a stabilising "elevator" in flight, with a small vertical rudder for lateral guidance.

### *Conclusion*

Just a word or two in conclusion. Though it be true that Cayley, unlike other pioneers of mechanical inventions, "put nothing on the market," I do not think we should allow this fact to detract from his merits. After all, to Shakespeare at least, the conception and cultivation of an idea was of far greater significance than any deeds which might spring therefrom. Moreover, though I find it difficult to assess the influence of Cayley's experiments and theoretical writings, it may surely be allowed that his ideas were in the direct line of true progress, and led to ultimate accomplishment. It has been said by an authoritative French writer, Alphonse Berget, that Cayley's name deserves to be recorded "in letters of gold at the beginning of the history of the aeroplane." Whether it might not fitly be so written at the beginning of the modern history of aeronautics is a matter of opinion. In any case it will, I think, be generally agreed that it deserves to be writ large and in imperishable ink on the roll not only of England's, but of the world's great aeronautical pioneers.

Cayley's aeronautical writings, as referred to in the foregoing "Notes," are:—

1. On Aerial Navigation (Mechanical Flight), contributed to Nicholson's "Journal of Philosophy," Vol. XXIV., 1809, pp. 164-174; Vol. XXV., 1810, pp. 81-87 and p. 161, etc.
2. On Aerial Navigation (Dirigible Balloons), Tilloch's "Philosophical Magazine," Vol. XLVII., 1816, pp. 81-86 and 321-329; also Vol. L., 1817, pp. 27-35.
3. Practical Remarks on Aerial Navigation (Dirigible Balloons), "The Mechanics' Magazine," Vol. XXVI., 1837, pp. 418-428; also (in the same publication) Retrospect of the Progress of Aerial Navigation, pp. 263-265, and On the Principles of Aerial Navigation, pp. 273-278, both in Vol. XXXVIII., 1843, and both dealing with Mechanical Flight.
4. Mémoire sur le Vol Artificiel (Wing-Propelled Gliding), "The Bulletin of the Société Aérostatique et Météorologique de France," No. 4, 1853, pp. 147-151.

The three papers in Nicholson's Journal (No. 1) were reprinted in the Aeronautical Society's Annual Report for 1876, and again as Aeronautical Classics, No. 1, 1910; also in Means' Aeronautical Annual for 1895. The "Practical Remarks" (No. 3) were reprinted in "Aeronautics," Vol. II., 1909, p. 142, and Vol. III., 1910, p. 1.

*Wilbur Wright Lecture, May 31st, 1923*

# RELATION BETWEEN AERONAUTIC RESEARCH AND AIRCRAFT DESIGN

BY JOSEPH S. AMES,

PROFESSOR OF PHYSICS, THE JOHNS HOPKINS UNIVERSITY; CHAIRMAN, EXECUTIVE COMMITTEE, NATIONAL ADVISORY COMMITTEE FOR AERONAUTICS (U.S.A.).

It is a great honour to be invited to give the Wilbur Wright Lecture on Aeronautics, especially so for a fellow citizen of the Wright brothers. I think that I appreciate the honour all the more because of personal relationships with Mr. Orville Wright and because, since the day of their first successful cross-country flight, I have had the opportunity of realising the truly unique qualities of these great men. The fact cannot be emphasised too often that, from the very beginning of their work, their point of view was that of the scientific investigator. Empirical methods, engineering development, did not satisfy them; they wished to know the underlying scientific facts, and to build on them. They had, in reality, the true concept of the purpose of the great aerodynamic laboratories of to-day.

The selection of a subject for the Wilbur Wright Lecture is not an easy matter, especially when the selection must be made months in advance and when, as in this case, the request was made to send the title at once by cable. I confess my title is banal, but it was the best I could think of which would be sufficiently indefinite to allow me to include in the lecture the results of several investigations then in progress. For there is always a grave uncertainty in any physical investigation as to the day when the results obtained will have sufficient value to be reported.

The aerodynamic laboratory with which I am connected is the Langley Memorial Laboratory, not far from Old Point Comfort, Virginia, which has been developed since 1915 by the National Advisory Committee for Aeronautics of the United States. This Committee is an independent Government agency, not under any of the Departments, but reporting directly to the President. We have a laboratory for power plant investigations; a large wind tunnel of the type developed by the N.P.L.; another tunnel in which the air may be compressed to twenty atmospheres or more; excellent facilities for the design and construction of instruments; and a large fleet of aeroplanes equipped for scientific purposes. In addition, we are able to engage the services of competent mathematical physicists familiar with aerodynamics. What we would like to do would be to give free scope to these latter, and to conduct the laboratory tests under their direction, so that theory and knowledge of facts could make progress together. But this is not possible in an establishment whose primary purpose is to give advice to other Governmental services, especially advice concerning questions raised by these services. It is true that we can often inspire these questions, and we can always, in the process of obtaining the answers, learn more than is required for the specific purpose. It follows, that while we are conducting practical tests we are also doing fundamental scientific work continuously, exactly as a justice of a high court expresses his deepest thoughts as *obiter dicta*.

As it has happened, two problems of a general nature have come to us this year from both the Army and the Navy, which, while not new at all, have led to new methods and to new knowledge. Both have an immediate bearing upon the design of aircraft; and it was for these reasons that I selected my rather indefinite title for this lecture.

The first problem stated generally was to learn more about the distribution of forces on the parts of aircraft. It came to us in three questions:—(*a*) How is the distribution of load over a wing tip and aileron modified by changing the plan form of the wing of an aeroplane? (*b*) Why are high-speed pursuit aeroplanes subject to certain types of accident, such as the ripping off of the linen envelope of the wings? (*c*) What are the forces to which the fixed and movable surfaces and the envelope of an airship are subjected when it is making manœuvres?

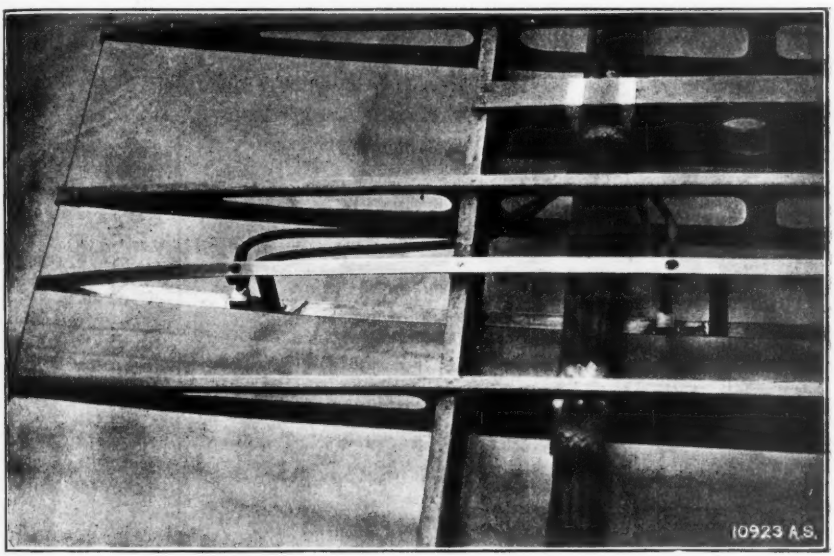
The first of these led to an extensive investigation in the standard wind tunnel. One series of tests was on four model aerofoils without ailerons, having square, elliptical and positively and negatively raked tips; the second series was on wings having raked tips with ailerons adjusted to different settings. The models had a chord of six inches and a mean semi-span of 18 inches, and the method of images, recommended in one of the British R. and M. reports, was adopted in the investigation. A large number of series of openings were made

*Thomas Morse MB-3 ready for wing and aileron pressure distribution tests.*

in the surfaces of the aerofoil, and each was connected to a liquid manometer. The results give a great deal of what is apparently new information concerning the air flow near the tip of a wing. They will soon be published both in tabular and in graphical form, so that designers can calculate with ease the distribution of lift between the ends of the wing spars, the shears and bending moments, and the aileron efficiency. Further, with the knowledge obtained, proper distribution of load in sand testing is facilitated. The most important general conclusions are that tips with a positive rake give an erratic distribution of lift near the tip of the aileron and that this may be avoided by the use of a negative rake. Considerable new light is also thrown upon the question of aileron balance. (Several lantern slides were shown.)

In order to study the air-flow about a high-speed pursuit aeroplane, a Thomas-Morse MB-3 machine was rebuilt and suitably prepared for experimentation. This has a maximum air-speed of 145 m.p.h. A large number of holes were made in the two surfaces of both the upper and lower wings; these were connected by rubber tubes to recording multiple manometers mounted in the fuselage; so in this way sixty records could be made simultaneously.

The manometer, which has been described in published reports of the Committee, consists of a series of metal capsules across the middle of each of which is stretched a metal diaphragm. In most of the tests the two holes facing each other on opposite sides of the wing were connected to the opposite sides of the capsule; but in some cases only one hole was so connected, the other side of the capsule being joined to a reservoir in the cockpit communicating with a static tube whose opening was in the interior of the wing. Special attention was paid to the distribution of pressure in the slipstream and near the leading and trailing edges. Since there is such a great variation in pressure over a wing, each capsule was adjusted separately so as to have the proper sensibility corresponding to the opening with which it was connected. At the leading edge pressures as high as 200 lbs./sq. ft. had to be measured, while further back the pressure often did not exceed 30 lbs./sq. ft. An accelerometer, a recording air-



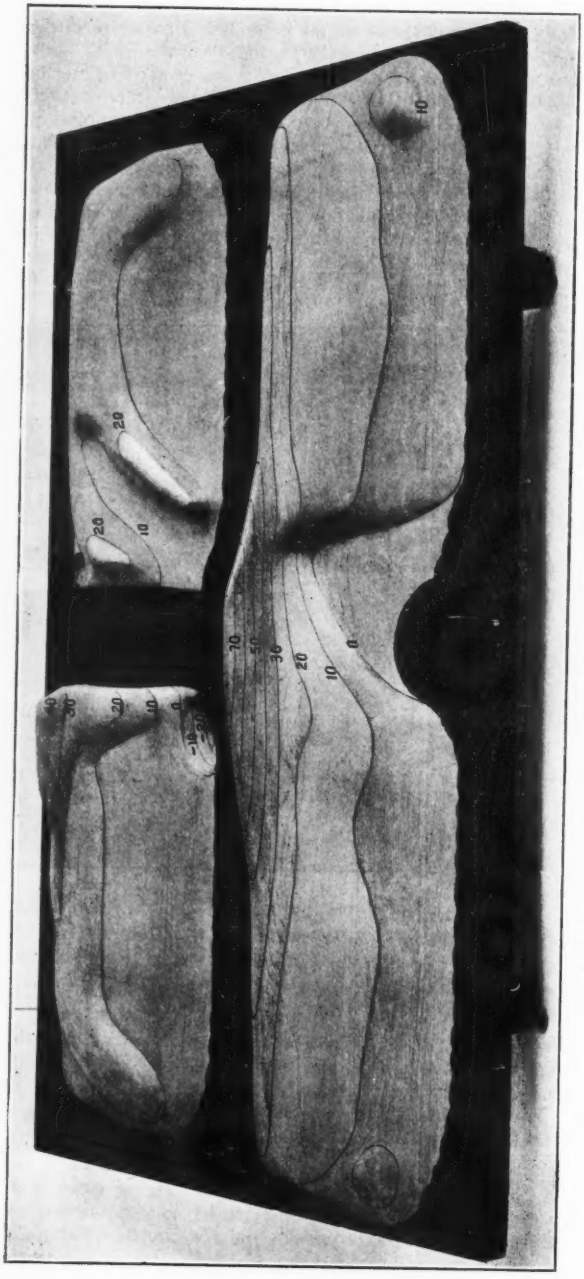

*Enlarged view of portion of skeleton wing of MB-3, showing tubes and surface connections for pressure distribution tests.*

speed meter, a control position recorder, and an electric chronometer were also installed in the aeroplane.

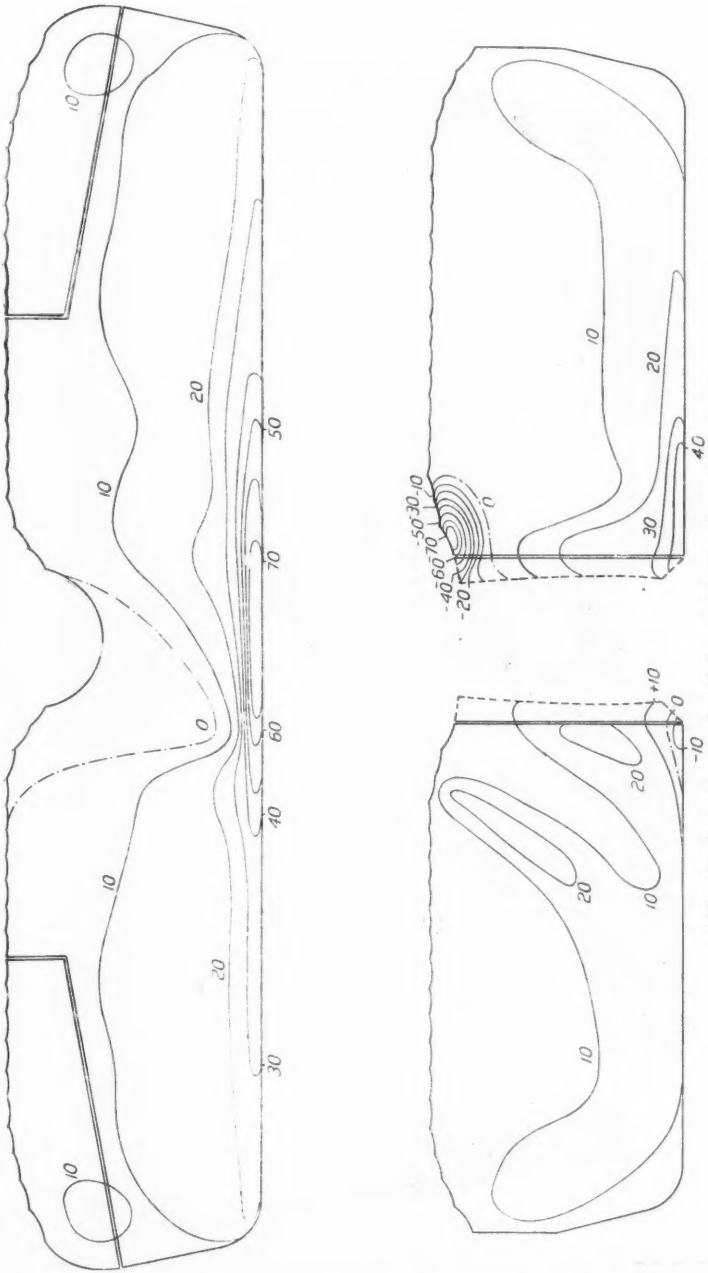
The information specially desired was the distribution of lift over the portions of wings in the slipstream during steady flight and that over the entire wings during violent manœuvres. Measurements were made at air-speeds of 70, 115 and 145 miles per hour at closed, medium and full throttle under conditions of steady flight, and also during three manœuvres, a roll, a flattening out of a dive and a vertical bank at 150 m.p.h.

The result can be understood most easily by the use of graphical methods. Contour lines of pressure may be drawn on a model of the wings; or, what is far more striking, three dimensional models may be constructed. Both these methods are illustrated.

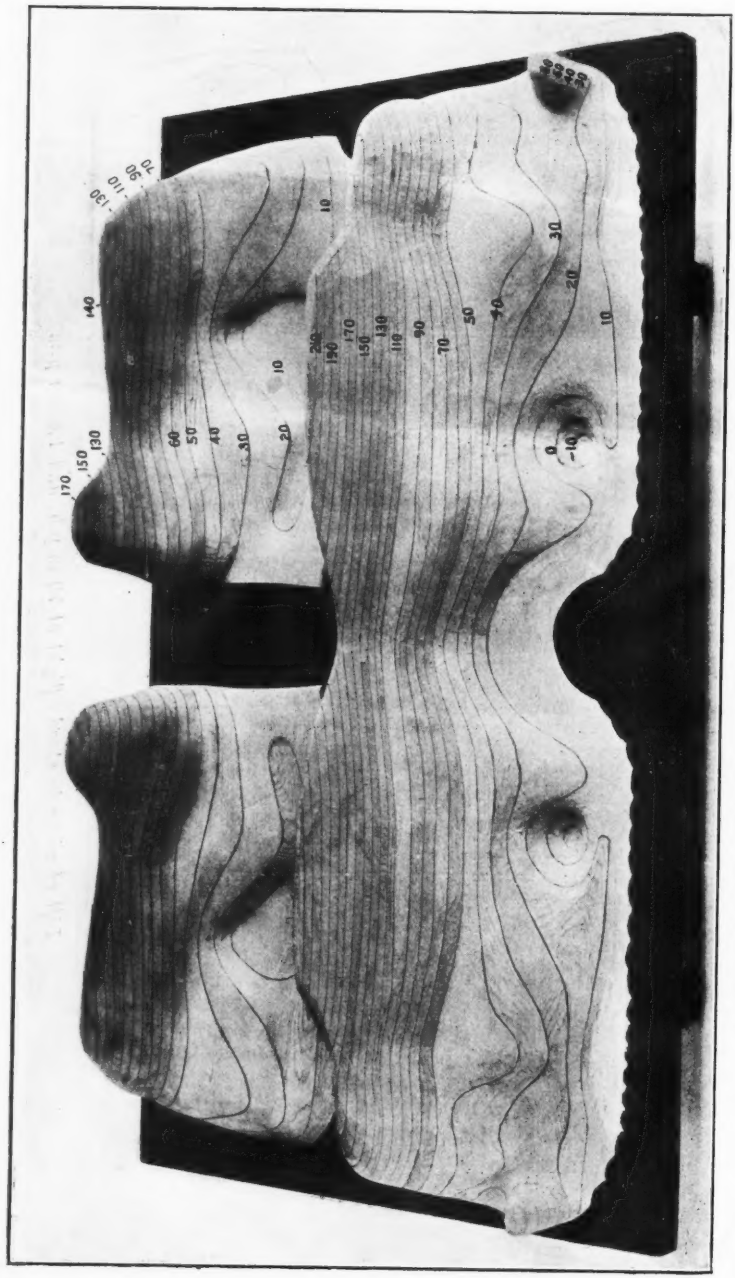
The numbers adjacent to any contour line indicate the total pressure upward




*Model in relief, showing lift of MB-3 wings in steady flight at 70 m.p.h. and 1,600 r.p.m.*




*Lift of wings in steady flight at 70 m.p.h. and 1,600 r.p.m.*




*Model in relief, showing lift of MB-3 wings in a vertical bank at 150 m.p.h. and 1,900 r.p.m. Acceleration 4.2 g. Elevator pulled up 19°.*

in lbs. per square foot, *i.e.*, the combination of the effects on the two sides of the wing. The relief maps also give the combined effects.

Some of the most striking facts observed are :—

1. The lift in the slipstream during steady flight is far from uniform on this aeroplane; at high air-speed and high engine-speed a lift of 100 lbs./sq. ft. was observed on the leading edge of the upper wing, while on the leading edge of the lower right wing there was an area of down pressure of 60 lbs./sq. ft.

2. At low air-speed and high engine-speed, that is while climbing, there was at the trailing edge of the lower left wing, near the fuselage, a down pressure of 70 lbs./sq. ft.

3. When the suction on the upper surface of a wing was measured with reference to the air inside the wing, it was found to amount to as much as 76 lbs./sq. ft. in steady flight, whereas in one isolated point an inward pressure of as much as 24 lbs./sq. ft. was observed.

4. In flattening out of a dive the wings support only 80 per cent. of the total load on the aeroplane, whereas in a vertically banked turn at 150 m.p.h., where the acceleration rose to 4.2 g. the wings carried 90 per cent. of the load, the remainder being borne by the fuselage and tail surfaces.

5. In steady flight at 145 m.p.h. the lift per sq. ft. of the upper wing is twice that of the lower, the total lift of both wings being about 400 lbs. greater than the weight of the aeroplane, balancing the down load on the fuselage and tail. This fact is, no doubt, due to the rigging of this particular aeroplane, *i.e.*, to the angular difference between the wings and to the lower wing being almost at zero lift.

It is important to add that this MB-3 machine is a single-seater, so that the pilot has to control the machine and press the button which starts all the automatic recording devices. This investigation of the MB-3 proved so interesting and offered so many suggestions that further studies of pursuit aeroplanes have been called for; the plans are now perfected for similar investigations of the latest types of military fighting aeroplanes. One problem in this connection is to compare the inherent advantages and disadvantages of monoplane and biplane machines.

As is well known, the United States is interested in the construction of airships. The Navy has practically finished a large rigid, and the Army has well under way a semi-rigid. As is equally well known, the actual scientific knowledge of the aerodynamics of airships is not extensive. At the request first of the Navy and later of the Army, our National Advisory Committee undertook to study and report upon the airship designs made by these two services. In connection with this work one of the technical staff of the Committee, Dr. Munk, elaborated a certain theory of the airship which was distinctly novel but led to results at variance with accepted practice. It was evident that real knowledge could be obtained only by extensive experimentation on actual airships. What was needed primarily was a series of measurements of pressures over the envelope and surfaces of an airship when in steady flight and when making manœuvres. For this purpose a non-rigid airship, Navy type C, was placed at the disposal of the Committee. It is 200ft. long, 40ft. in diameter, and has 200,000 cubic feet capacity. Pads were specially designed for the measurement of pressure. These lie practically flush with the envelope of the airship, and each consists essentially of a metal box whose top and bottom surfaces are pear-shaped, roughly 2ins. by 4ins., and held a distance of one-hundredth of an inch apart by means of studs; in the top plate there are grouped in a comparatively small circle 22 holes each three-hundredths of an inch in diameter; a brass tube $\frac{1}{4}$in. in diameter serves as an outlet from the box. This is connected by rubber or aluminium tubing to a liquid manometer in the car of the airship.

*and 1,900 r.p.m. Acceleration 4.2 g. Elevator pulled up 19°.*

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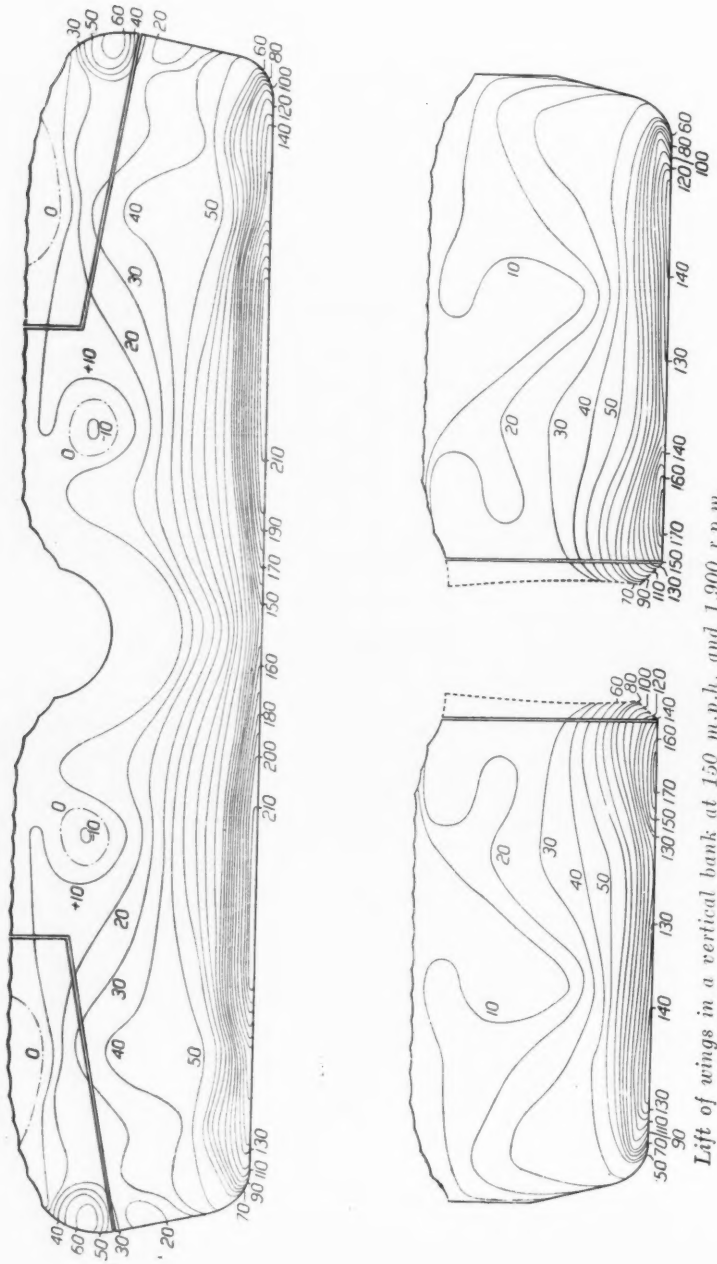
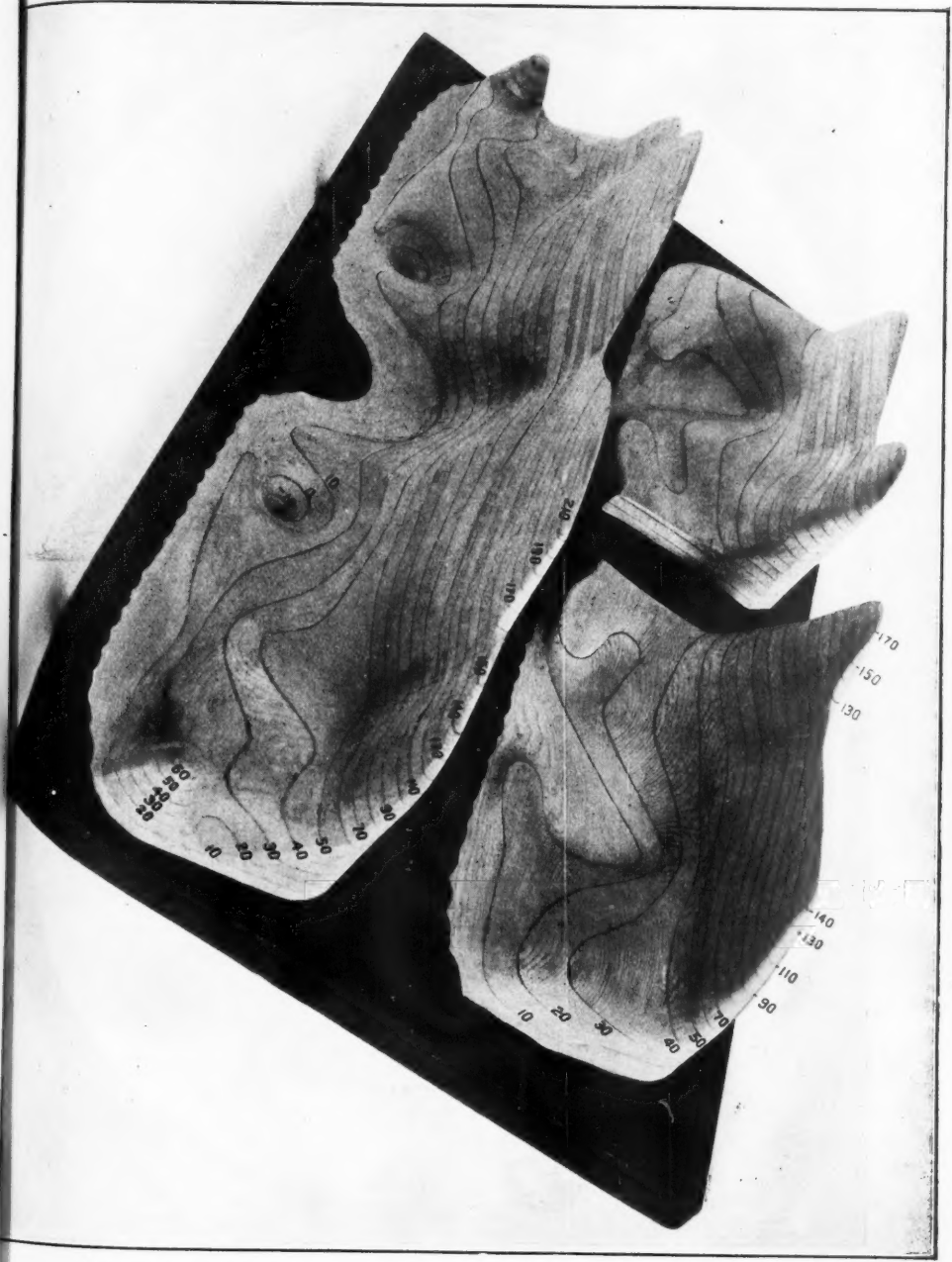



*Lift of wings in a vertical bank at 150 m.p.h. and 1,900 r.p.m. Acceleration 4.2 g. Elevator pulled up 12°.*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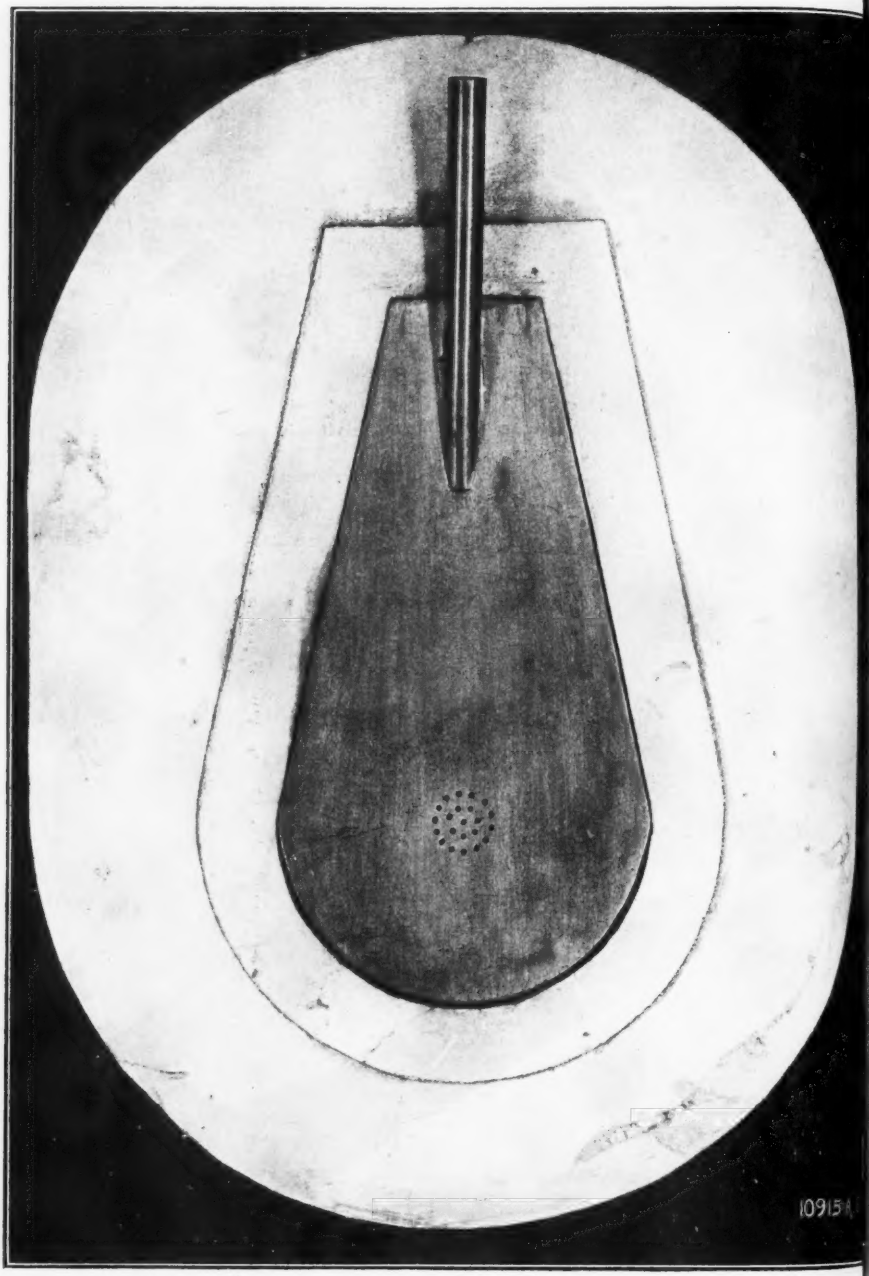
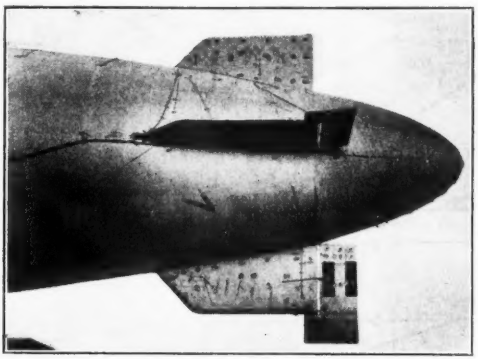


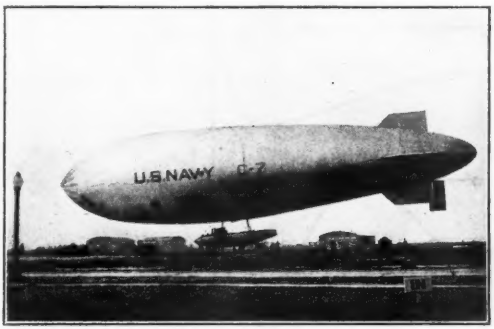
*Pressure pad for use on airships.*

There are about 400 of these pads on the envelope and surfaces of the airship, thirty-six being in the bottom fin and rudder. Simultaneous reading of 260 manometers may be made photographically.



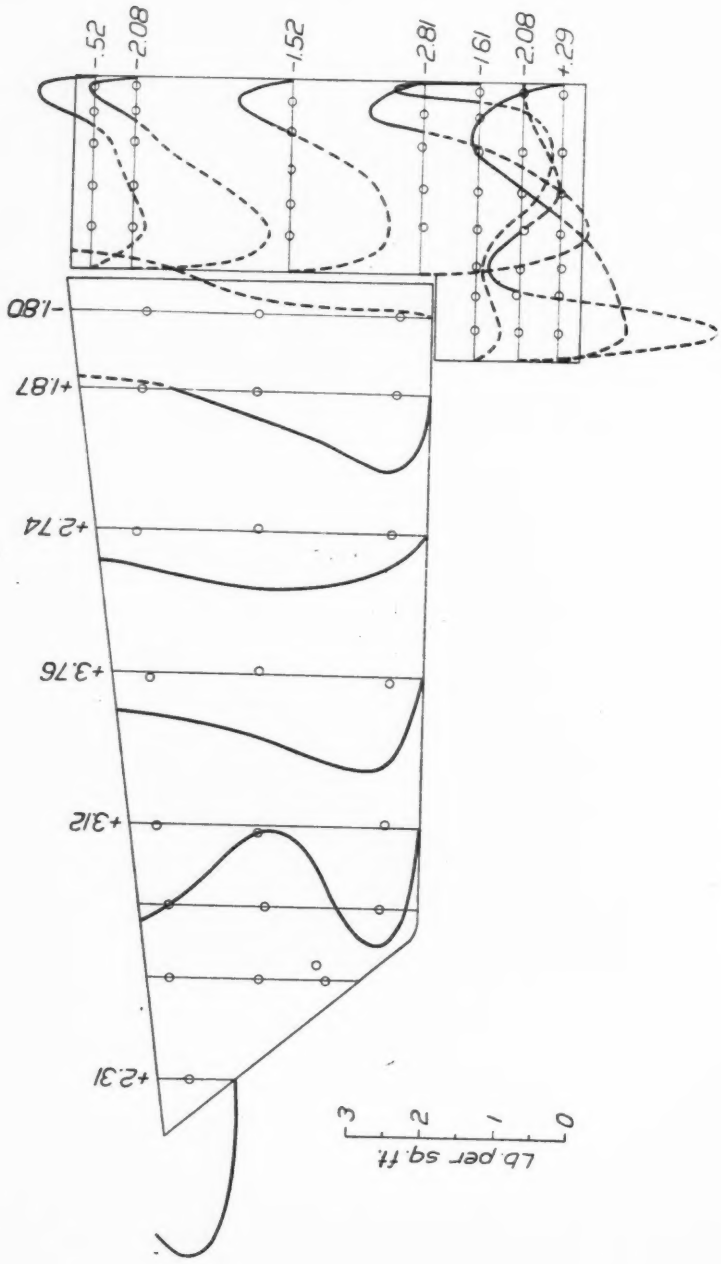
*Control surfaces of airship, showing location of pressure pads.*

This investigation of the aerodynamics of an airship is not yet completed, but I can show you certain observations which indicate the importance and novel character of the results being obtained. One illustration shows the pressure distribution over the bottom fin and rudder in circling flight, and the other when the airship while in steady flight has its helm put hard down.

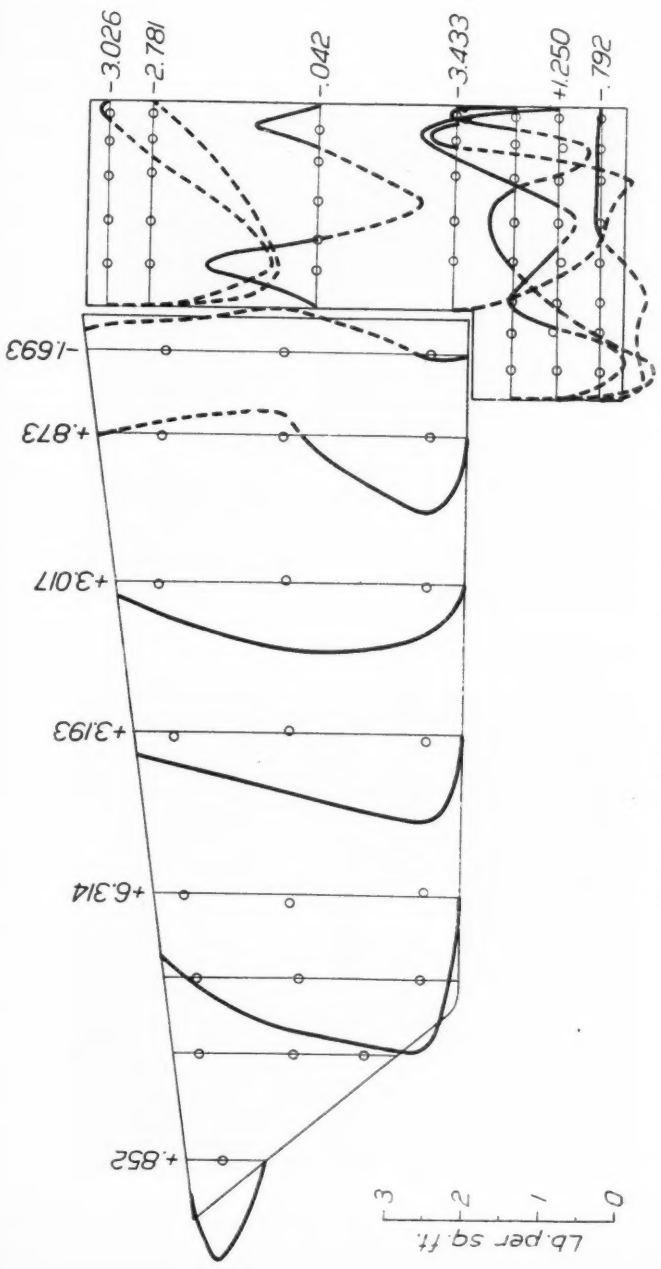



*"C" class airship on which the pressure distribution work was done.*

The drawings do not require much explanation, but emphasis may be placed upon the results shown when circling flight is begun. When the helm is suddenly applied, and before the airship attains an appreciable angular velocity, the angular acceleration creates such a large force on the vertical fins in the opposite direction to the force on the rudder that the net force on the stern of the airship is much smaller than has been supposed hitherto. It follows that the condition of the sudden application of the rudder is not a serious one from the point of view of the stresses in the hull of the airship. Presumably the reversal of the helm, when the airship is in a steady turn, does not cause a large increase of the bending moments beyond those already existing in that condition.



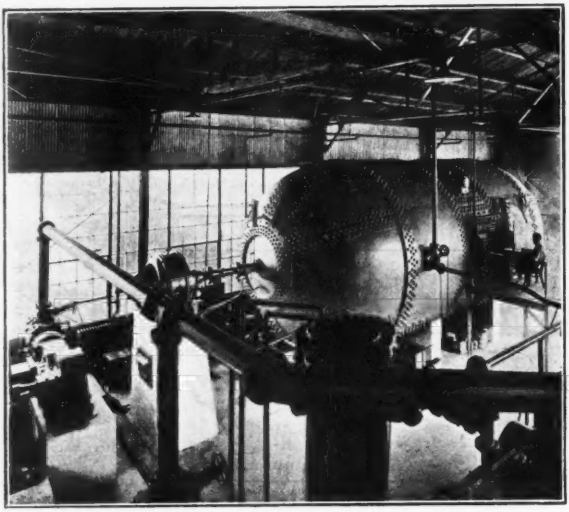

*Pressure distribution over bottom fin and rudder. Circling flight*



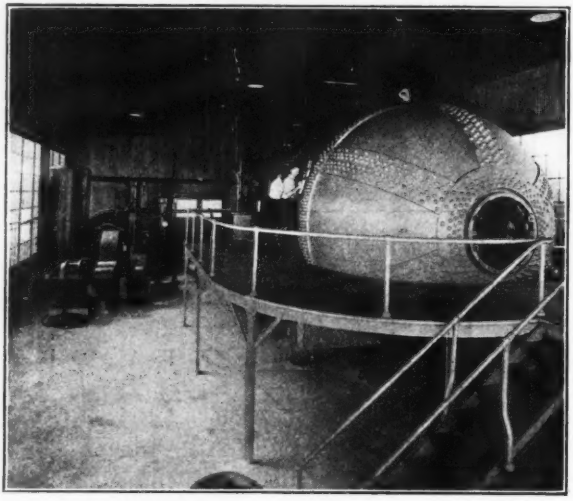

*Pressure distribution over bottom fin and rudder.*
*Beginning of circling flight.*

Pressure distribution over bottom fin and rudder. Circling flight

There are the three problems referred to at the beginning of this paper as requiring an elaboration of the methods for the study of pressure distribution; and no one can question the importance of the results obtained in the proper design of aircraft.



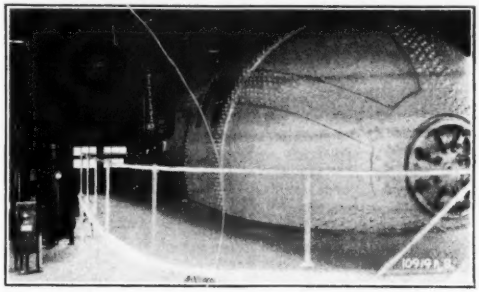
*View of compressed air wind tunnel, showing fan motor and air pipes.*



*Compressed air wind tunnel with observation platform and compressor.*

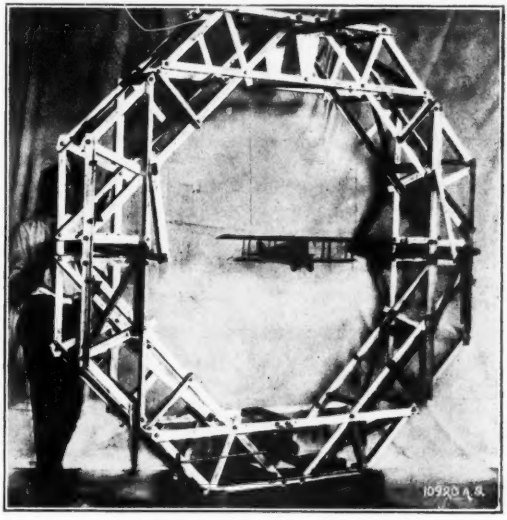
Quite a different set of questions has been asked our Committee, which lead in the end to an investigation of the so-called scale effect. Certain questions can,

of course, be answered on theoretical grounds, and answered definitely; but the great majority cannot. Any aircraft is a complicated mechanism made up of many parts; all of these have definite aerodynamical characteristics; but from a knowledge of these we cannot pass to that of the machine as a whole. The



*Wind tunnel No. 3, showing observation platform and desk.*

question as to the changes in forces and moments with scale, especially in manœuvres, is exceedingly difficult. The first investigation which should be made on scale effect is to determine which aerodynamic properties are most susceptible to the effect; after that, the number of problems to be undertaken is practically infinite.



*Balance for wind tunnel No. 3.*

At Langley Field our Committee has facilities for studying scale effect by four different methods, two of which are, I believe, unique. We have an ordinary wind tunnel, having a 5ft. throat and fitted with fans so that an air-speed of 100 m.p.h. (147 feet per second) may be used; this gives a certain Reynolds number, not very large. A larger number may be obtained by a free flight method

in which a large model is suspended below an aeroplane in steady flight; we have perfected methods for suspension and measurement, and the results are, on the whole, satisfactory. To secure a still larger Reynolds number, the Committee has had constructed during the past year a wind tunnel to operate with air compressed to 20 atmospheres or more. The tunnel proper is 5ft. in diameter at the experimental chamber, and is enclosed in a cylindrical tank with hemispherical ends. The walls of the tunnel are hollow, providing an annular dead air space in which the balance mechanism is installed. This may be controlled automatically, or settings may be made by small electric motors, operated from outside, which attach or release heavy balancing weights by means of cams, or shift lighter weights along balance arms. The model is attached to the balance by wires, there being three balance arms for measuring lift, drag and pitching moments. The tank is 35ft. long and 15ft. in diameter, and weighs 83 tons. It is mounted on a concrete foundation and is partially surrounded by a working platform. An observer on this makes settings and readings by looking into the tank through small glass windows. The density of the air in the tank is controlled by two compressors driven by electric motors. Continuous stages may be secured from one-tenth of an atmosphere to twenty atmospheres. Circulation of air is effected by a two-blade propeller of special design, 7ft. in diameter and driven at 900 r.p.m. by a 250 h.p. synchronous motor mounted on a separate foundation outside the tank. The drive shaft is made tight against air leakage where it passes through the head of the tank by a loosely packed gland, through which oil is circulated.

The concept of such a tunnel was originated by Dr. Max M. Munk, and this particular one was designed by him; and the mechanical equipment was designed and installed by Mr. D. L. Bacon, both members of the Staff of the Committee. The latter is in charge of the operation of this tunnel as well as of other tunnels in the Committee's laboratory.

It may be of interest to note that when the tunnel is operating at its greatest density, it is equivalent in scale to a tunnel 100ft. in diameter running at 60 miles per hour. It takes about an hour and a half to "inflate" the tank fully.

Another method for obtaining a large Reynolds number, which is used by the Committee, involves the accurate measurements of the motion of an actual aeroplane in flight. To this end the Staff of the Committee have perfected a large number of recording instruments. Among these may be mentioned a single-component accelerometer; a three-component accelerometer; a three-component angular velocity recorder; a control-position recorder; a control-force recorder; an air-speed meter; an angle of attack recorder, and an electric chronometer. The Committee owes the design of these instruments to the exceptional ability of two of its staff, Mr. F. H. Norton and Mr. H. J. E. Reid.

The latest instrument developed and one used in work about which I shall speak later is a form of kymograph.

It consists of a streamlined body, shaped like a bomb, from the front end of which projects a N.P.L. pitot tube, and which has a tail appendage to render the whole directionally stable. There is a transverse shaft through the centre of mass, to the two ends of which are attached suspension wires leading to winches in the cockpit of the aeroplane, so that when the latter is in flight the kymograph may be lowered to a distance of 25 feet so as to be in undisturbed air. In the upper forward surface of the "bomb" there is an opening closed with a cylindrical lens, outside of which is a small vertical mirror, so that the rays of light from the sun may be reflected through the lens and then through two crossed slits on to a photographic film. The pitot tube is connected to a capsule manometer, whose motions are recorded on the same film. This is wound on a drum, inside of which is a constant speed electric motor driven by a current led in through the suspension wires of the instrument. An actual photograph of the records on the drum is given.

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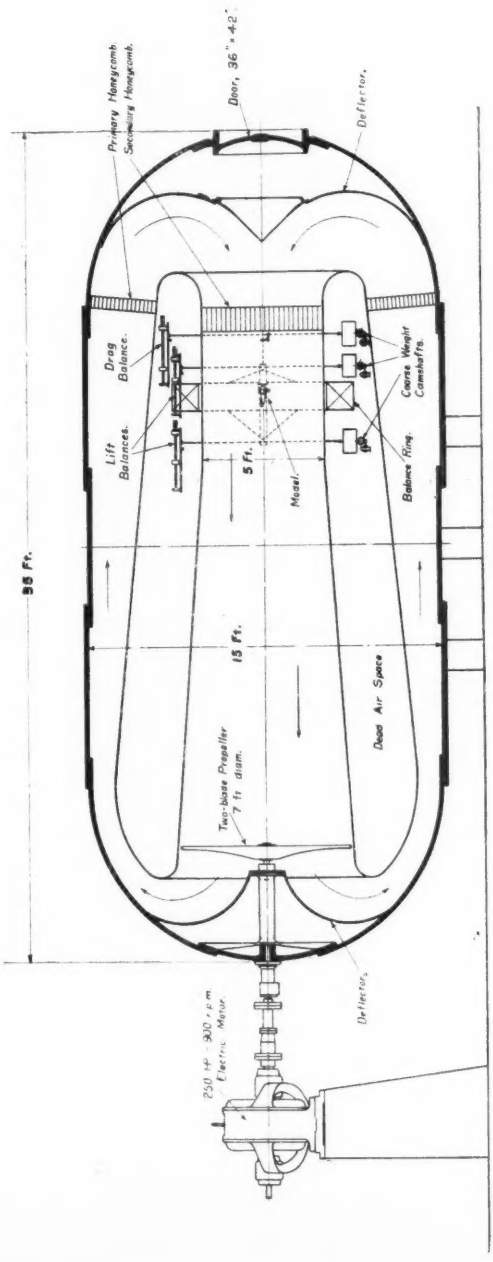
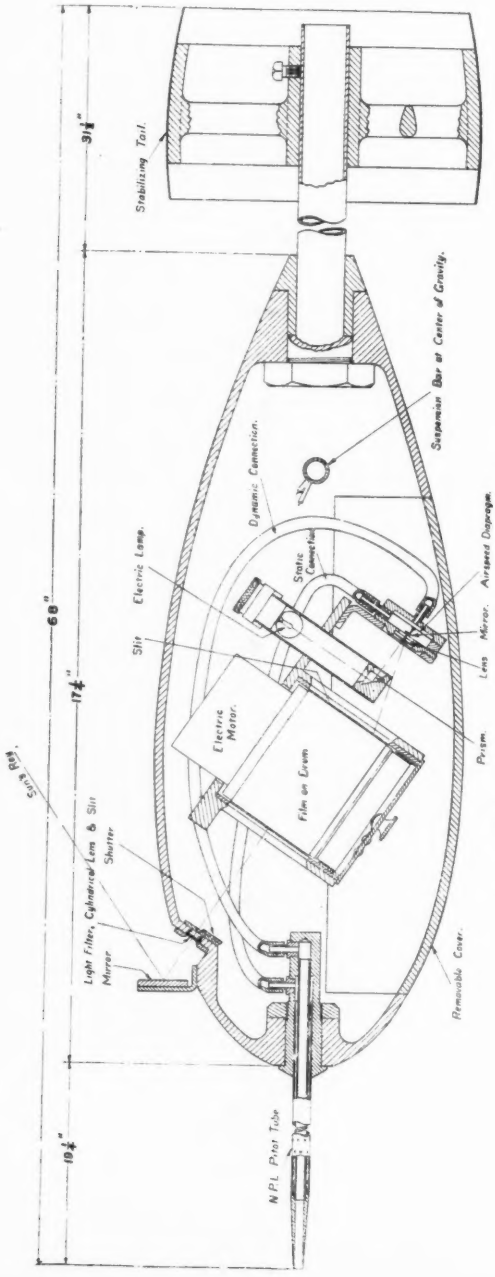


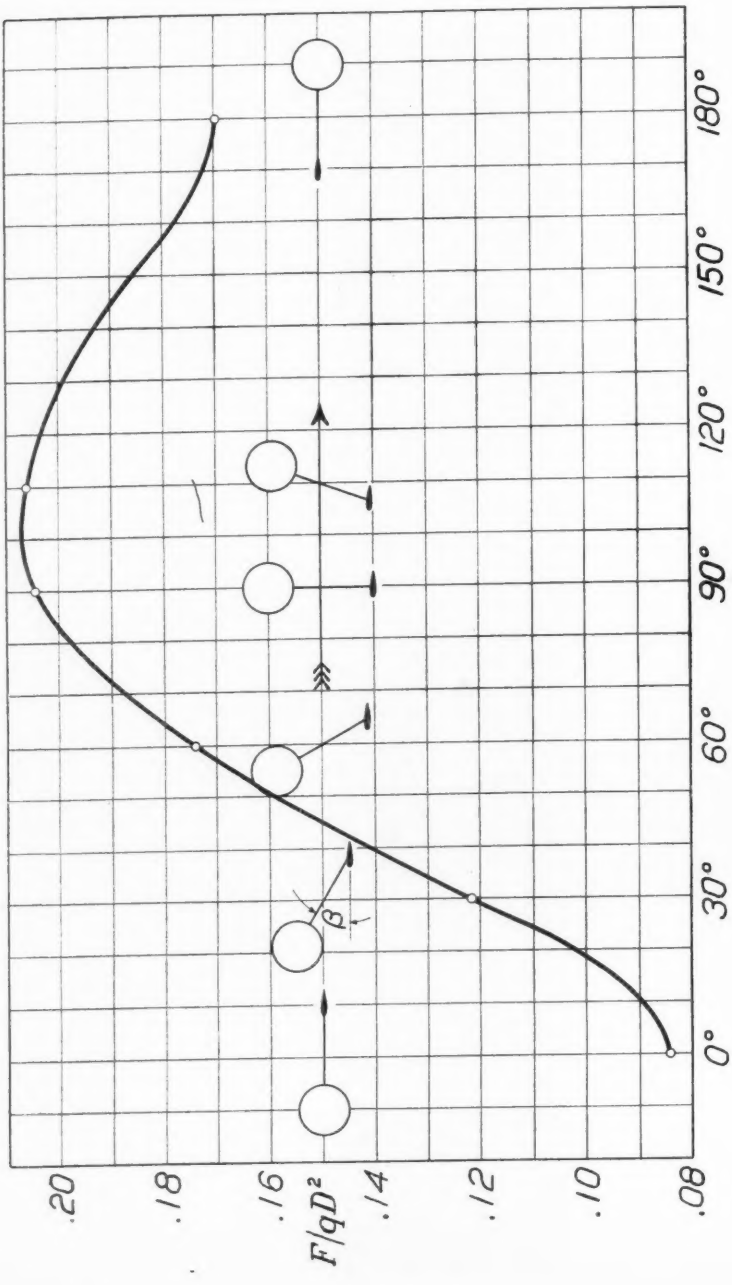

*N.A.C.A. variable density wind tunnel.*




*N.A.C.A. trailing Kymograph-airspeed meter.*

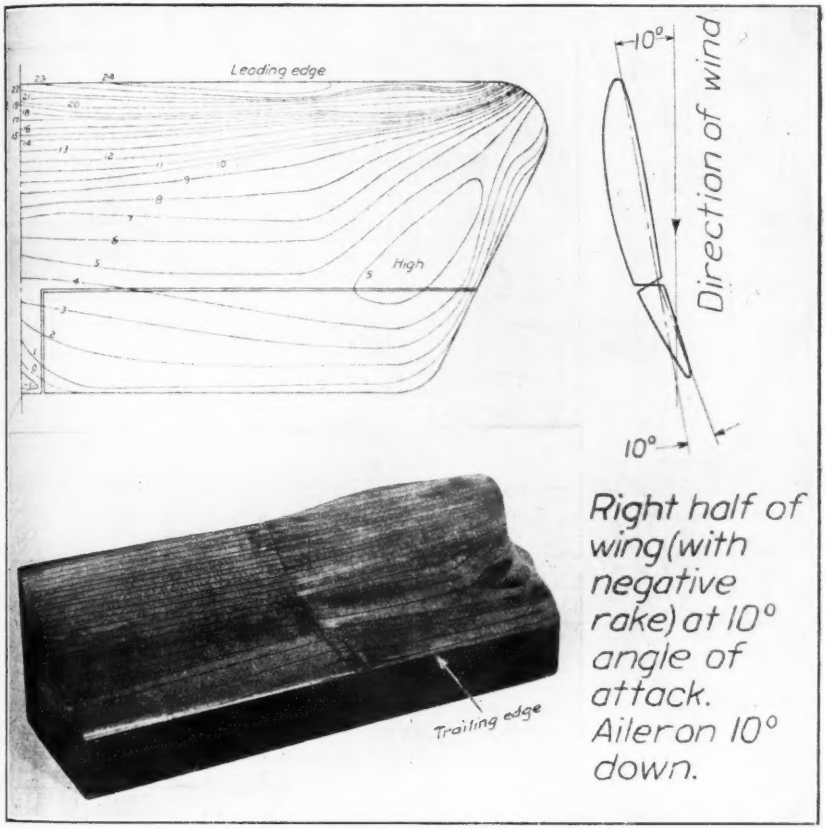
*Kymograph Record.*

*Airspeed records are the sharp curves. Sun records are broadened.*




*Effect on resistance of sphere of direction of support with reference to airstream.*
*Reynolds Number is $1.5 \times 10^5$.*

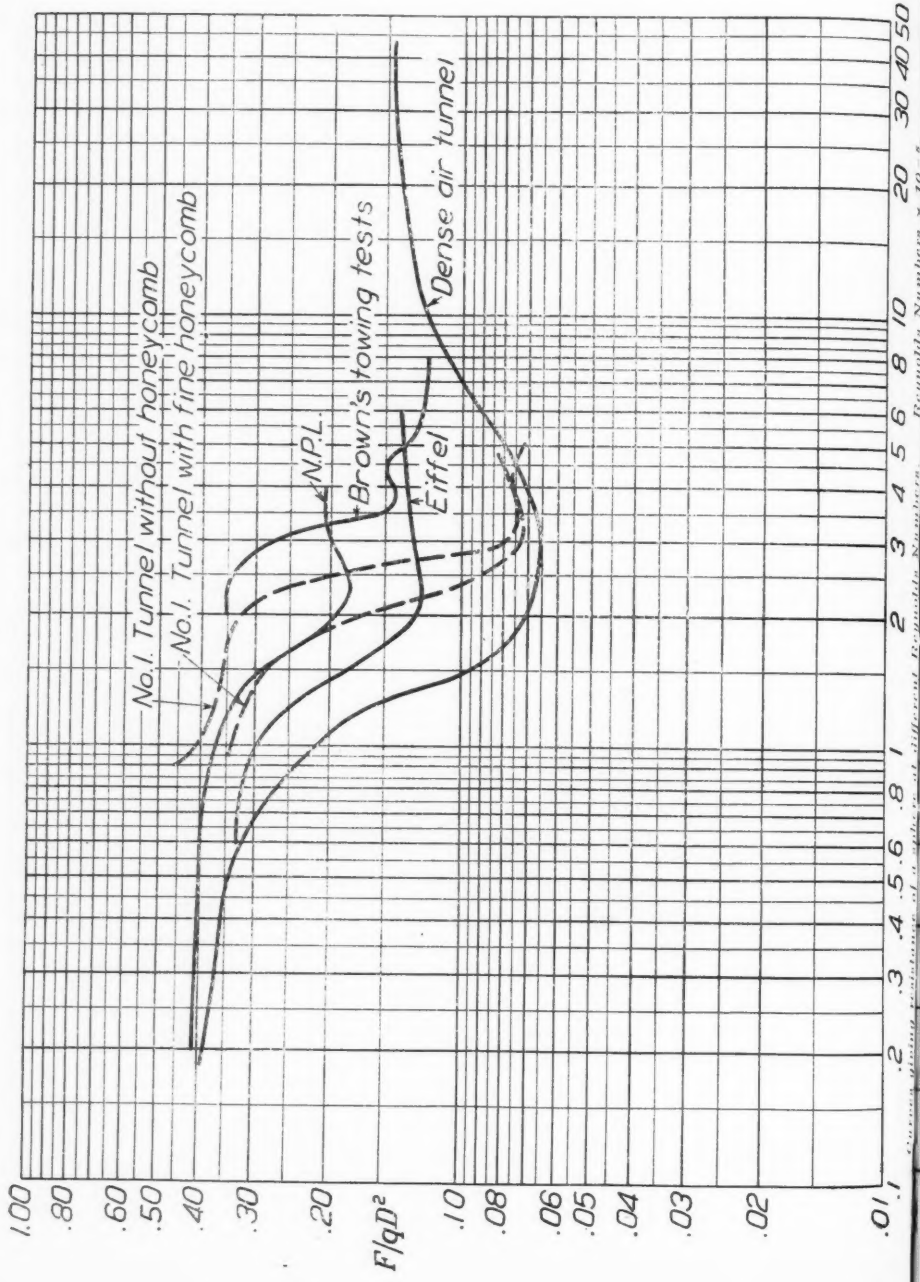
When the aeroplane is flown in a direction away from the sun, the kymograph takes a position along the direction of the relative wind, and a continuous record will be made of the angular position of the sun with reference to this direction. An observer on the ground observes simultaneously the altitude of the sun; and so one obtains a record of the angle between the flight path with reference to the air and a horizontal line. The air-speed is measured at the same time, as is also the angle of attack of the aeroplane itself. Therefore, if gliding flights are




*Plan and elevation showing contour lines and built-up model.*

taken, values of the ratio of lift to drag may be measured at various angles of attack at known air-speeds. This method is obviously independent of vertical air currents. As an illustration of its accuracy, a chart is shown giving the values of angle of glide with reference to air-speed at different values of $V/ND$ in which $V$ is the air-speed, $N$ is the number of revolutions per second of the propeller, and $D$ its diameter. By a preliminary model investigation it was found that the value of $V/ND$ was 1.02 for the condition of zero torque. These, and all other "free flight" tests under the direction of the Committtee, have been carried

*Effect on resistance of sphere of direction of support with reference to airstream. Reynolds Number is $1.5 \times 10^5$.*


No.1. Tunnel without honeycomb
No.1. Tunnel with fine honeycomb
N.P.L.
Brown's towing tests
Eiffel
Dense air tunnel
F/qD²
Reynolds Numbers × 10⁻⁶
1.00
.80
.60
.50
.40
.30
.20
.10
.08
.06
.05
.04
.03
.02
.01
.1
.2
.3
.4
.5
.6
.8
1
2
3
4
5
6
8
10
20
30
40
50

$\frac{V}{ND} = 1.34$
$\frac{V}{ND} = 1.02$
$\frac{V}{ND} = 0.77$
Angle of glide
10°
8°
6°
4°
2°
0°
50
60
70
80
90
100
110
120

out by Mr. F. H. Norton and Mr. W. G. Brown with the aid of the Committee's most skilful test pilot, Mr. Thomas Carroll.

With these facilities at the Langley Memorial Laboratory, it is hoped that rapid progress will be made in the elucidation of the scale effect problem.

Unfortunately for the purposes of this paper, the compressed air tunnel was actually put into daily operation for observation purposes only about the first week in April, and so I can report the results of only two series of tests. For this reason, although I have no cause to question their accuracy, they should, I think, be regarded as provisional.

The first scale effect measurements undertaken were on spheres. There is nothing novel in this problem, but some of the results are interesting. Spheres of various sizes were studied in the two tunnels, with their supporting spindles in the direction of the airstream and at various angles to it; other spheres were towed suspended at a considerable distance below an aeroplane in flight; and finally certain spheres were taken aloft by an aeroplane on particularly quiet days and allowed to drop, their motion being determined by theodolite observations from the ground. The results of all of these methods are given on the accompanying diagram.

This test was undertaken both to obtain large Reynolds numbers and to investigate the condition of turbulence in the new wind tunnel. If time were available, I would call attention to several interesting features of these curves.

The second test on the subject of scale effect was made with reference to a type of aeroplane using thick wings and having small parasite resistance. A Fokker D-7 was selected for this purpose. An aeroplane was equipped with suitable apparatus, and a model of one-fifteenth scale was made which was fitted with its proper propeller. Series of measurements on models and in full flight have been made; the aerodynamic characteristics of lift and drag were measured at different attitudes, and the results obtained are shown in the accompanying diagram.

If the use of these scale effect methods justifies our present hopes, we shall be able in a comparatively short time to place at the disposal of the designer of aircraft a wealth of information which should increase markedly the accuracy of his work.

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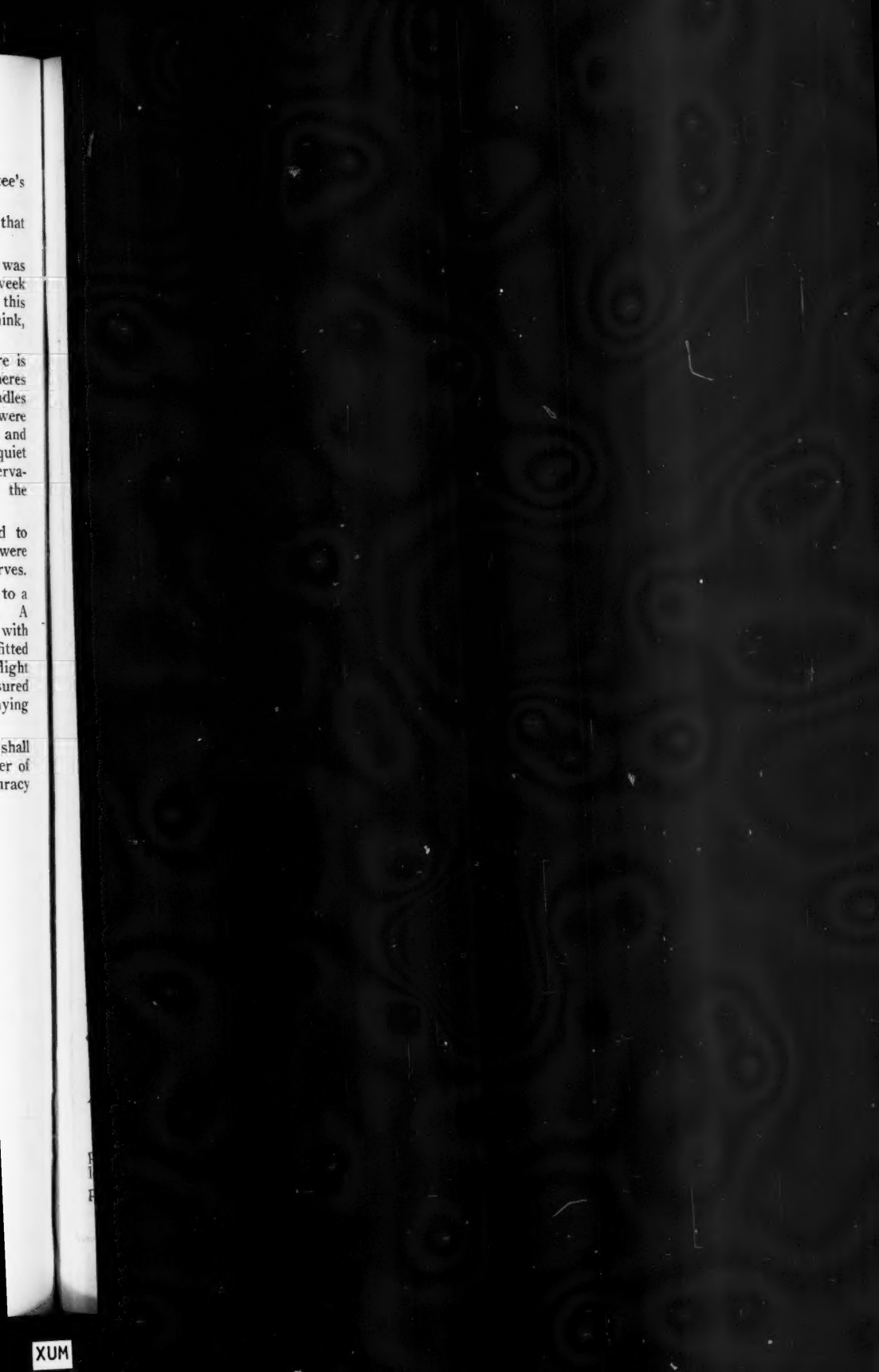
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